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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Estimation of forest structure metrics relevant to hydrologic modeling using coordinate transformation of airborne laser scanning data" *by* A. Varhola et al.

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Reviewer 1 – Konrad Schindler

We greatly appreciate Dr. Schindler's careful attention to our study. While generally positive and supportive of our manuscript, the reviewer has made a number remarks which are related to more nuanced discussions about alternative approaches to our method and future work on testing hydrologic models rather than to the technical soundness of the current article. We have carefully answered the reviewer's questions below and incorporated clarifying sentences in the revised version of the article, which

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have increased its quality.

GENERAL COMMENTS

1) "... the hydrological relevance of the improvements reached in the paper remain speculative, since they are not tested". REPLY: The paper is not focused on directly testing the effects of the derived metrics on hydrologic modeling because the method developed to derive LAI and SVF –two of the most important model parameters– from coordinate-transformed ALS data required substantial analyses and discussion. A manuscript containing both the development of this methodology and interpretations of hydrologic nature would be considered too long and would not allow an in-depth examination of the novel methodology to generate synthetic hemispherical images from ALS. A follow-up study recently submitted to another journal will show how to extrapolate these metrics to the wider landscape before they can be input to fully-distributed hydrologic models (the ultimate goal).

2) "The description of the model and experiments (in sections 2 and 3) is rather terse and at times lists variables and numerical results without much interpretation." REPLY: The reviewer seems to be more specific about this concern in his "Specific Comments" section, where his remarks are addressed.

3) "While I think that the capability to estimate per-point GF rather than its average over large areas is a strength of the work, that capability is not evaluated, if I understand correctly (section 2.6 'The resulting average GFs are compared...')" REPLY: We consider our original modeling exercise calibrating the synthetic fisheye photos with their real counterparts on an individual basis to actually test the capacity of our method to estimate GF at the point-level. Our statistical approach also includes a leave-one-out type of procedure which confirms model accuracy when predictions are made on independent individual points. The plot-level averages described in section 2.6 are designed to show the overall benefit of using synthetic hemispherical images as opposed to simple vertical metrics (which actually require a minimum horizontal area to be computed).

Since plot averages are commonly used by many hydrologic studies, their evaluation (section 2.6) was complimentary to our point-level analysis and was needed to justify our method when comparing it to other studies (e.g. Solberg et al., 2006).

4) "More importantly, it remains unclear whether that point-wise information is actually needed. First of all, do models exist which can exploit the more local gap fraction (GF)? Second, if yes, do these models give better predictions than the conventional ones?" REPLY: Physically-based process equations within hydrologic models are commonly developed at the point or small plot levels. This is particularly true for radiation extinction simulations that are associated to sub-daily solar paths that determine the amount of energy that reaches the ground. Due to the interaction between canopy geometry and local solar paths, this energy is specific for a particular point underneath the canopy equivalent to the camera position if hemispherical photography is used, or a pyranometer location if radiation is measured directly. Pomeroy and Dion (1996) provide a good example of how point-level measurements of canopy structure and solar radiation are used to parameterize hydrologic models. Point-level information is not necessarily "better" than information from other scales, but it is required to construct a detailed understanding of hydrologic processes. The article already discusses the advantages of our point-level methodology on P5536-L21-23 and P5554-L8-19. However, to address the reviewer's remark, we have added that a direct input of point-level forest structure metrics in hydrologic models able to analyze processes at this scale is an additional advantage in the last paragraph of the introduction.

5) "…And third, do the more accurate GF estimates make a difference compared to those derived without the coordinate transformation, i.e. does the improvement observed in the paper persists in an end-to-end evaluation of the hydrological model? Some evidence about that last question, what the added value of the proposed methodology is for the actual hydrologic modeling, would really strengthen the paper, even more since the scope of the journal is hydrology." REPLY: See reply to comment 1) above; additionally, it must be mentioned that the ultimate goal of remote sensing is

to improve the characterization of forest structure at the watershed scale so that metrics can be input in fully-distributed models (P5555-L1-4). This article is a first step to obtain the metrics that are directly relevant for current hydrologic models at any location within ALS data. A follow-up article already submitted to a hydrology journal addresses numeral 3) of the future work section of the current article (P5555-L1-4) by correlating data from thousands of synthetic hemispherical images from ALS and Landsat spectral indices in order to extrapolate the relevant metrics to the landscape. In other words, generating synthetic ALS images does not improve hydrologic modeling per se because conventional hemispherical images can be used for this purpose. Our advantage, as explained on P5554-L15-19, is that now we can generate LAI, GF and SVF on any location of ALS data, which later allowed us to obtain frequency distributions of these metrics at the watershed level. Neither this article nor its follow-up have quantified the benefit of these new remote sensing-based methodologies on hydrologic modeling efficiencies since that is considered and extensive line of future work. The reviewer is welcome to request our follow-up study, now under review, for further clarifications.

6) “In its current form the paper has one major conceptual issue: the somewhat unnatural strategy to massage the measurement data into a model-compliant form, rather than adapt the models to the observable data. It is not clear why such a strategy is chosen and the paper does not enter that discussion. ‘Changing the data rather than the model’ happens at two levels: - on the lower level, the work gives the impression of bending over backwards to transform ALS points such that they can serve as input to the GLA software package, which is tailored to hemispherical images. Why not instead adapt or replace GLA, and develop a method of estimating the correct gap fraction directly from ALS data? The new geometry after transformation is the one GLA expects, but it is not a natural way to represent the information contained in ALS data.” REPLY: Producing synthetic hemispherical images from discrete ALS is a novel approach that has not been tested before, and is one of many methods that researchers could use to estimate GF, LAI, etc. The main reason why we chose our particular strategy

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is because hydrologists are very familiar with hemispherical photography as the simplest way to obtain forest structure metrics. To address the reviewer's concern, we have added the following clarification in section 4.4 (Methodological advantages and applicability): "We have chosen to generate synthetic hemispherical images from ALS readily available for processing with GLA because hemispherical photography has become one of the most popular methods to obtain GF and associated forest structure metrics, and its outcomes have been systematically used to parameterize hydrologic models (e.g. Woods et al., 2006; Ellis and Pomeroy, 2007; Ellis et al., 2011). Alternative approaches might produce different versions of the same metrics that can vary enough in magnitude to invalidate current models without alternative calibrations."

7) "...- on a higher level, one might ask why one would actually want to compute the output values delivered by GLA. If the actual goal is hydrological simulation, and the desired measurement technology is ALS, then it would seem natural to adapt the hydrological model in such a way that it accepts structure parameters derived from ALS, rather than those defined by a different (previous) technology." REPLY: See our reply to point 6) above. Hemispherical photography and software packages like GLA or Hemiview have been developed for decades through a vast body of studies to become one of the strongest ways to characterize vegetation structure and radiation transmission (P5534-L9-24). Any method that uses ALS directly would need to overcome the limitation related to the fact that only a minimum fraction of the canopy structure is <sampled> by such a technology. Calibrating the metrics resulting from ALS synthetic hemispherical images with those from optical counterparts through empirical relationships is only a means of reconstructing the full extent of canopy structure. However, we agree with the reviewer at the most fundamental level in that we need to explore more direct methods and consider that a new era of research in hydrologic modeling should use alternative metrics derived from ALS, TLS and even spectral indices from satellites to develop entirely new process-based equations for radiation transfer, precipitation interception/evaporation, etc., although this is way beyond our dataset's capabilities. This thought has been included in the first paragraph of section 4.5 (Future work) to

address the reviewer's concern. While hydrology would benefit from direct parameterizations with remotely-sensed variables, our study is of value because it will later determine the benefit from inputting fully-distributed structural metrics on the models. In other words, before testing new variables, we need to quantify the improvements with high-resolution versions of the current variables distributed at the watershed level (follow-up article). This topic and the persistent dependency of hydrologists on traditional LAI are explained in detail on P5555-L11-26.

8) "In fact the authors acknowledge that fact, but then do not go all the way. Although the proposed method is good at replicating HP, the GF has to be converted to LAI for further processing, because that is the out-of-the-box parameter the models expect. There is a very valid and interesting discussion of the fact that GF contains the required information and might be a better proxy for canopy structure than the contentious LAI. However, the proposed work is sitting between the chairs in that respect: it does (for understandable reasons) not go so far to estimate LAI, which is the parameter the current models want, but then if you decide that the models should move to different proxies for forest structure, why stop at GF and not come up with one that fit better with ALS?" REPLY: The only reason why we are not able to test new metrics to parameterize models is because we would need detailed measurements of above- and below-canopy shortwave and longwave radiation, additional meteorological variables, instrumentation to measure snow canopy interception, sublimation, etc. Physically-based equations within hydrologic models are complex and require intensive measurements to be developed properly. A new sentence has been added at the end of section 4.5 (Future work) to comment on this: "Since producing alternative physically-based models requires detailed measurements of above- and sub-canopy shortwave and longwave radiation, precipitation interception and evaporation, snow accumulation and depletion, among others, new studies are required re-parameterize hydrologic models to substitute LAI with GF or other metrics directly obtained from remote sensing and quantify the resulting benefits/losses."

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SPECIFIC COMMENTS

9) “The paper limits itself to discrete return LiDAR. This might be dictated by the available data, but nevertheless is a rather heavy limitation in terms of practical relevance. In airborne laser scanning full waveform data will soon be the norm, and it is very likely to have significant advantages for the task at hand, to estimate forest structure. Thus the development of LiDAR technology might very quickly render the technical details of the work obsolete, and at least a discussion of the potential of full waveform ALS would be in order.” REPLY: The application of the methodology with other LiDAR datasets and particularly full waveform data (FW) has been raised by other reviewers. For this reason, this sentence has been added in the fourth paragraph of section 4.3 (Modeling strategy): “Of particular interest is the application of this method to full-waveform (FW) LiDAR data, which will be increasingly used in the future and provides a more detailed profile of canopy elements and additional radiometric information (Pirotti, 2011). FW LiDAR also has the potential to assist in a better estimation of the return dimensions by the analysis of target backscatter cross sections (Wagner et al., 2006, 2008). However, given that discrete ALS has been used extensively in many regions, our methodology is not likely to become obsolete in the near future.”

10) “Statistical significance testing is over-used for my taste. A significance test without a known loss function (which in turn defines the specific significance level that should be used) says nothing about the importance of an effect, see for example the well known [Ziliak and McCloskey 2007]. Low significance is a sign that the noise level is high compared to the number of samples, so there might be small-sample bias. But it does not solve the question whether an input variable is important (i.e. ‘how big is big enough to matter’). Also, Table 5 seems to confirm there is something fishy here – in what situation would a correlation of 0.01 and one of 0.93 both be significant?” REPLY: First of all, there was a typing mistake in Table 5 and the 0.01, indeed not significant, has been corrected. The entire table was double-checked thoroughly and this was the only error identified. While we are aware of the controversies regarding

significance testing, our calibration models comply with the established techniques and assumptions of linear regression and have proven to provide very reasonable results, as shown on Table 6.

11) "In general I am not sure about the necessity to exclude variables in the specific context - has it been verified that they actually hurt if left in? On the other hand in section 3.1. the size of geometric element "affected the slope" - that is important, since it influences the calibration, but it is not further elaborated." REPLY: Our view is that if a variable is not contributing to explain significant portions of the variation in a model, they must be left out to favour parsimony. Yes, it was thoroughly checked if there was a benefit in including all variables in the models and the results showed that the exclusion of variables identified as not statistically significant had a negligible impact on the quality of the models (P5548-L14-19). Regarding the effect of return size on the slope of the calibration (also mentioned in P5541-L16-20), the last sentence of section 3.1 (Preliminary sensitivity analysis) was improved: "...varying the minimum size of projected returns is important for the calibration relationships because it affects the r2 via changes in slope (a 45° slope would maximize r2), but will not significantly influence RMSE due to a lack of discernible scatter reduction (Fig. 2)."

12) "A technically debatable decision is to render LiDAR points as opaque spheres. Why not use some smarter ray-tracing method? There are several methods readily available in the computer graphics literature. Since vegetation is better described in a volumetric way, volume rendering would seem like a plausible candidate that could almost certainly improve the resulting images. As a side effect, that could also be a possible way to account for biases due to the vertical viewing direction." REPLY: We agree with the reviewer that a number of alternative approaches could have been developed to obtain the same or better results as our methodology. As explained before, any technique would have to deal with the fact that ALS is providing a truncated point cloud representation of canopies that must be completed in some way to reproduce the real detail of tree elements. One of the main advantages of our methodology is sim-

plicity, and portraying ALS returns as opaque spheres was supportive of this. Lengthy discussions about alternative approaches is beyond our scope but we have included your suggestions as a list on the second-last paragraph of section 4.2 (Physical representation of canopy elements with ALS).

13) “Going further, an open issue remains whether the problem could not also be solved through improved LiDAR-based metrics. The chosen approach works, but it should not be portrayed as "the right way". Maybe the ‘Standard ALS metrics’ are simply the wrong features for the job and one could derive alternative metrics from the LiDAR points without going through hemispherical images? Figure 6 proves that the standard metrics are bad, but it does not prove that the coordinate transformation is the optimal way of mapping the raw data to a gap fraction.” REPLY: We have not intended to portray our method as “the right way”; on the contrary, we have dedicated a significant portion of the discussion to explicitly acknowledge our limitations. Figure 6 proves the potential improvement incorporated by projecting ALS returns in a spherical coordinate system, but only future studies can determine which is the optimal method.

14) “A purely technical question I have is, what motivates the restriction to linear regression? It does not make sense to me to declare this an a priori design decision (section 2.5). A linear model is only good if according to the data the relation is in sufficiently good approximation linear, and Fig. 2 suggests it is not: in all three plots (d,e,f) a concave function would fit better, so I have doubts that a linear fit is an ‘appropriate tool’.” REPLY: The reviewer is absolutely right, so the first paragraph of section 2.1 (Regression modeling) has been reworded not to give the impression that linear relationships constitutes an a-priori assumption. The original text resulted from preliminary analysis showing that non-linear relationships were not present in the full dataset (note that Figure 2 is based on a small subsample), and hence resulted in the wording that was now modified. After receiving this reviewer’s comment, we double checked if non-linear models would provide better results and confirmed they don’t. “Appropriate tool” was substituted by “suitable” on the second paragraph of section 4.3 (Modeling

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strategy).

MINOR TECHNICAL CORRECTIONS

15) "Section 4.1 says 'we expect...little impact' of the geometric distortion, which is not backed up by evidence. We all have to make such educated guesses at some point, but in that particular case it would be easy to test empirically. The ground truth and the infrastructure for creating the images is already there, so I would think it is not too much work to run an experiment." REPLY: The reviewer's suggestion was undertaken and a subsample of 20 images of all stand types with and without geometric distortion was generated. It was confirmed that the difference in LAI of mature and medium stands was negligible (less than 1

16) "The problems with young vegetation (Figure 3, BRC1-A3) suggest that the proposed method would benefit from a prior on the radial distribution of the gaps. From the real images one can observe that from vegetation regions there is 'almost always' vegetation radially outwards to the nearest image border, which makes sense since under the top of a straight pine tree there must be a stem. That could be added (even in a stochastic way if required) in the rendering procedure to improve the images (also those of BRC2-C1 and VOD2-C4 show unrealistic disconnected clusters in the middle)." REPLY: As explained above, there are a number of approaches that can be followed to create a more realistic representation of canopy elements before or after they are projected in a polar coordinate system. We have deliberately chosen to maintain the ALS data as raw as possible without adding any 'inexistent' information to reconstruct virtual stems or branches. The logic behind this is that the method will remain simple and easy to apply to other datasets by keeping the number of adjustable parameters low. Furthermore, what the multiple regression calibration does is exactly that: absorb a major portion of the inconsistencies between synthetic and real images and yield reasonably accurate predictions without the need to alter the canopy's representation (P5552-L5-8). If these calibration models are statistically valid and produce good predictions, as they do, then there is no technical flaw with the methodology. Also, note

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that tree reconstruction techniques might vary among different stands, and one major advantage of our approach is that it can be applied directly to all stand types with the same models. This advantage was added in section 4.4 (Methodological advantages and applicability) of the revised manuscript.

17) “Voxelisation is only mentioned briefly as a possible alternative - I believe it would simplify things a lot. It is also better suited for fuzzy occupation/transmission values (volume rendering). Maybe one could try at least a quick proof-of-concept?” REPLY: Our article is lengthy and we wish to concentrate on discussing our own methodology rather than alternative approaches.

18) “The geographical registration is mentioned as a limiting factor for accuracy. Why is that? With conventional surveying methods (differential GPS, tachymeter) one should be able to get at least decimeter accuracy?” REPLY: Our registration problems originate from the fact that the exact hemispherical camera position was not recorded when the pictures were taken (at that time our study had not been planned), and the camera could have been located at any point within 1 m of permanent stakes which were installed in each plot. This clarification has been added in the first paragraph of section 2.2 (Data acquisition for modeling).

19) “Typos: p5540 l18 ‘the each projected ALS point’; p5551, l3-4 ‘needs to sufficient enough’; p5552 l5-6 ‘Any shortcoming is unavoidable’.” REPLY: Corrected and rephrased.

20) “Equations: some geometric relations are rather cumbersome to read as verbatim text, it would be much clearer to give an equation. In particular, this applies to p5535, l34-24; p5540, l19-22.” Reply: the first equation is from another study so the reader can refer to Solberg et al. (2006) to review it, especially because we prefer to avoid equations of secondary importance in the introduction. For the explanation of P5540-L12-22, the equation has been included in the text and a new illustration of how it works has been added (new Fig. 3a).

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21) “In a similar spirit, I think most readers would appreciate a graphical sketch instead of the explanation on p5541, l1-10.” REPLY: Agreed; see new Fig. 3b.

22) “A slight inaccuracy appears in section 4.4 when the authors claim as a methodological advantage ‘avoids...separation of ALS points into ground and non-ground classes’. That is not true. To place the virtual camera you need the DTM, and that is generated by filtering out the non-ground points.” REPLY: Agreed; avoiding the separation referred to using all returns in the projected images, but it can lead to this confusion as also pointed out by another reviewer.

23) “The claimed advantage to ‘avoid voxelixation’ should better be removed or justified. In my understanding that is a bit of an empty claim, since voxelization of a given point cloud is a trivial mechanical procedure done by a computer.” REPLY: Agreed; sentence removed.

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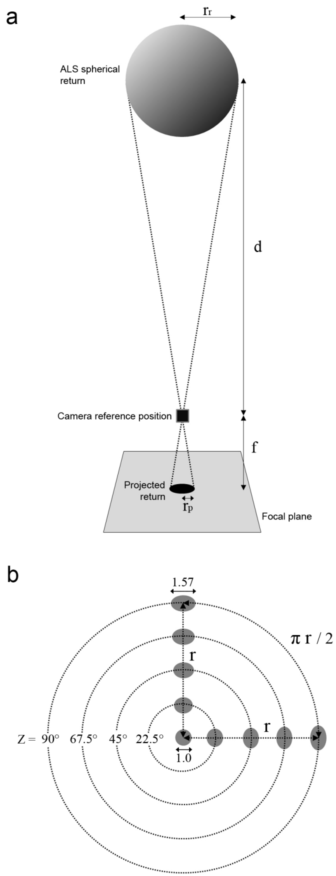

Fig. 1.

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